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CHOLERA.

Notes on the Pathology and Treatment of Cholera. By GEORGE JOHNSON, M. D., F. R. C. P., Professor of Medicine in King's College, etc. (Continued from p. 4.)

We have shown that two classes of facts stand in bold opposition to the theory that the collapse of cholera is due to a drain of liquid from the blood. We have seen that there is no such direct relation between the degree of collapse and the amount of liquid discharged from the blood as must exist if the hypothesis in question were true. We have also seen that the symptoms of collapse differ essentially from those which an excessive drain of fluid from the blood is known to produce. We have now to inquire *whether the effect of various and opposite modes of treatment upon the symptoms of collapse affords support to the theory that a drain of liquid from the blood is the essential or the chief cause of that condition.*

The effect of alcoholic stimulants.—The condition of a patient in collapse—cold and pulseless, and apparently exhausted—is one which naturally suggests the use of stimulants. Any one who has witnessed the speedy improvement in the pulse and other symptoms, which usually follow the administration of wine or brandy to a patient who is fainting from loss of blood or exhausted by excessive purging, might reasonably expect to obtain similar results from the same means in the collapse of cholera. Accordingly, stimulants have been given, and given freely and boldly; and the result has been a very general conviction that in the stage of collapse they are not only useless, but positively injurious. Again and again have I seen a patient grow colder, and his pulse diminish in volume and power, after a dose of brandy, and apparently as a direct result of the brandy. Dr. Gull (*Report on the Morbid Anatomy and Pathology of Cholera*, p. 185) states that, "although opium

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and diffusible stimuli—brandy, camphor, and ammonia—were useful at an early stage of the disease, as collapse set in, they not only failed to produce any favourable result, but often aggravated the symptoms."

Dr. Paine, who has given an admirable description of cholera and its treatment in 1832, writes thus of stimulants (*Letters on Cholera Asphyxia as it has appeared in New York*, p. 42): "We have seen no benefit from their liberal use, and it is even doubtful whether they contribute much in any quantities. It requires the conviction of experience, however, to enable us to abstain from their use, and to resist the impulse to apply them to the dying spark."

The very general conviction as to the worse than uselessness of alcoholic stimulants in the collapse of cholera is the more to be relied upon, inasmuch as it has been forced upon men's minds in opposition to preconceived notions and prevailing theories. The action of stimulants in the collapse of cholera being obviously very different from their influence upon patients who have been exhausted by the loss of blood-constituents, we infer that syncope and choleraic collapse are pathological conditions essentially different; and this conclusion is confirmed in a most striking manner by the effect of other modes of treatment.

The effect of venesection on the symptoms of collapse.—It is scarcely necessary to assert that no sane practitioner would think of abstracting blood from a patient who has been reduced to a state bordering on syncope by any of the common sources of exhaustion which have before been referred to. It is obvious that the loss of blood in such cases might be attended with perilous and even fatal results. But what has been the effect of venesection in not one or two, but in a large number of cases of cholera, and in the hands of many different practitioners? Has the effect of this treatment been such as to afford support to the theory that collapse results from loss of liquid? or has it been to add to the cumulative evidence which stands opposed to that theory? We will endeavour now to answer these questions.

It is in the writings of the Indian practitioners that the largest amount of evidence is to be obtained as to the influence of bloodletting in cholera. Scott makes the following remarks on this subject (*Report on Epidemic Cholera*, p. lviii.): "The abstraction of blood, unless as an antispasmodic, is a remedy so little indicated by the usual symptoms of cholera, that its employment in the cure of this fatal disease has afforded a signal triumph to the medical art. It requires no common effort of reasoning or reflection to arrive at the conclusion that, when the powers of life appear to be depressed to the lowest degree, the pulsation of the heart all but extinct, the natural heat of the body gone, and the functions of the system suspended and incapable of being revived by the strongest stimulants, the abstraction of blood might yet prove a remedy against a train of symptoms so desperate." "Few remedies," he says, "on a fair trial, have been more generally and unequivocally advocated than free bloodletting; and the most that has been urged against it is, that it is not always successful." He then quotes reports from medical officers furnishing very striking testimony as to the benefit of bleeding in cases of extreme collapse.

Annesley states (quoted by Scott, p. lx.) "In place of syncope being produced by bleeding, in the cases which I have treated, the pulse has invariably improved, and the feelings of faintness and debility disappeared."

Bell makes the following statement (*Treatise on Cholera Asphyxia*, p. 118): "The effect of bloodletting would indeed sometimes appear almost miraculous. A patient will be brought in on a cot, unable to move a limb, and but that he can speak and breathe, having the character, both to touch and sight, of a corpse, yet will he, by free venesection alone, be rendered, in the course of half an hour, able to walk home with his friends."

Rogers gives the following description of the effects of venesection by a medical man who was himself the patient (*Reports on Asiatic Cholera in Regiments of the Madras Army*, by Samuel Rogers, p. 259): "There was a sensation which I am at a loss to describe, as if my heart was ceasing to beat, and a dread of suffocation; this sensation was instantly relieved by bleeding, and I recovered immediately."

The following striking case is recorded by Sir Ranald Martin (*The Influence of Tropical Climates on European Constitutions*, 6th ed., p 349): "On visiting my hospital in the morning, the European

farrier-major was reported to be dying of cholera. I found that during the night he had been drained of all the fluid portion of his blood. His appearance was surprisingly altered; his respiration was oppressed; the countenance sunk and livid; the circulation flagging in the extremities. I opened a vein in each arm, but it was long ere I could obtain anything but trickling of dark treacly matter. At length the blood flowed; and by degrees its darkness was exchanged for more of the hue of nature. The farrier was not of robust health; but I bled him largely, when he, whom but a moment before I thought a dying man, stood up and exclaimed, 'Sir, you have made a new man of me.' He is still alive and well."

Now let me ask, is it possible to reconcile facts of this kind with the theory that the collapse of cholera results from a loss of the liquid constituents of the blood? If Sir R. Martin's hypothetical statement, that his patient "had been drained of all the fluid portion of his blood," were an accurate expression of facts, can we conceive it possible that he could have "made a new man" of him by abstracting largely the blood which remained in the vessels? I maintain that the numerous well authenticated instances of great and immediate and permanent relief by means of venesection in the collapse stage of cholera, are utterly and hopelessly irreconcilable with the hypothesis in question.

The influence of purgatives.—If the symptoms of collapse were due to the drain of liquid from the blood, and its escape by the intestinal canal, it would seem to be impossible that the symptoms of collapse should pass away while the drain of liquid by vomiting and purging is continually going on. It would seem, too, that the action of purgatives during the stage of collapse must greatly increase the mortality. I do not here propose to consider the merits of the purgative plan of treatment. I wish only to refer to the indisputable fact that there are on record numerous well authenticated instances of recovery from extreme collapse, while the intestinal discharges were encouraged by repeated doses of emetic and purgative medicine. And, further, I challenge the advocates of the theory which I am endeavouring to refute to refer to a single case of recovery from collapse in which the intestinal discharges have not continued, in a greater or less degree, while the symptoms of collapse were passing off. If the theory in question were a true theory, the cessation of the intestinal discharges must always, and of necessity, precede recovery from collapse.

During the early part of the cholera epidemic of 1849, all the cases of cholera admitted into King's College Hospital were treated by liberal doses of brandy and opium. Under this mode of treatment, the mortality was very great. The treatment was then entirely changed; brandy and opium were discontinued, and large quantities of salt and water were administered. The effect of this treatment was to excite frequent vomiting, and certainly not to check, but rather to increase, the purging; and the result was a much larger proportion of recoveries than under the previous mode of treatment. I had no share in conducting the treatment on that occasion; but I was greatly struck by the different effects of the two opposite modes of treatment. I was also deeply impressed by observing that, during that epidemic, the arrest of the purging by opiates was in several instances *followed* by the worst symptoms of collapse; and a painful question arose in my mind, whether the collapse in such cases was not a direct result of the arrest of the purging.

At the commencement of the last epidemic—that of 1854—I had arrived at the conclusion that the commonly received theory of choleraic collapse is erroneous. I had the chief charge of the hospital during the whole period of the epidemic; and I gave emetics and purgatives to all the patients who came under my care. I have since published full particulars of all my cases. I am convinced that in many instances I gave an excessive quantity of castor oil: yet the result was a mortality, to say the least, below the average mortality in cases of equal severity. During that epidemic, many cases of choleraic diarrhœa came under my observation—cases in which there were vomiting, bilious purging, and cramps. These were all treated by castor oil, without opiates. They all recovered; and not one case so treated passed into collapse. Several of the medical officers, pupils, and nurses, and a considerable number of patients who were in the hospital for other diseases, had the premonitory symptoms of cholera. All were treated in the same way, and all recovered. In contrast with this most satisfactory result

stands the fact that, during the previous epidemic of 1849, several inmates of the hospital, nurses and patients, having been seized with choleraic symptoms, and being promptly treated by opiates, passed into a state of collapse and died.

I see no way in which the facts here stated can be reconciled with the hypothesis that the worst symptoms of cholera result from the loss of liquid, and that the main object of treatment is to check the vomiting and purging.

The effect of injecting hot saline solutions into the veins.—It is well known that the injection of a hot saline fluid into the veins during the collapse of cholera has often been followed by great temporary relief. The pulse improves; the temperature rises; the countenance becomes natural; the voice recovers its strength; and, in short, all the worst symptoms speedily disappear—usually, however, to return with all their former severity within a very short time. The late Dr. Mackintosh, of Edinburgh, during the summer of 1832, injected the veins of 156 patients, of whom only twenty-five recovered. There are probably but few practitioners who now expect any practical benefit from this mode of treatment—few who would consider it right to repeat this experiment. But there are many pathologists who maintain that, since all the symptoms of collapse speedily disappear after the injection of a certain quantity of liquid into the veins, this experiment proves conclusively that the symptoms which previously existed must have resulted from the loss of a liquid similar in character to that which the operation restores to the blood. I believe, however, that the true explanation of the manner in which the hot saline injections afford the surprising temporary relief which they are acknowledged to have done, has been missed; and that, rightly interpreted, the results of this experiment afford as little support to the hypothesis that collapse depends on loss of fluid, as do the effects of other modes of treatment to which reference has already been made.

In a future communication, I will give what I believe to be the true pathology of collapse; and then I hope to explain the *modus operandi* of the saline injection into the veins.

Before attempting to give what I believe to be the true interpretation of the symptoms of cholera, I have thought it desirable to direct attention to some of the facts and arguments which are opposed to the commonly received theory. There is so much of apparent probability in the theory which explains the symptoms of collapse by the loss of the watery portion of the blood, that the practice of giving opium and other astringents to arrest the intestinal discharges will continue more or less, in spite of failure and disappointment, until it can be clearly shown that the state of collapse has an entirely different origin and cause from that which the theory in question assumes. —*Brit. Med. Journ.*, June 17, and Sept. 9, 1865.

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Cholera.—At the Academy of Sciences, M. Velpeau lately quoted the opinion of three medical men who were more or less in favour of sulphate of copper as a cure for cholera—Dr. Burq, Dr. Lisle, and Dr. de Prado; and added, that the medication in question had produced no marvellous results in Parisian hospitals. M. Chevreul offered some considerations on our actual knowledge of cholera. The cause of cholera is unknown; so also is its therapeutical treatment, or otherwise the prize Bréant would have been already adjudged by the Academy. The observations made as to the appearance of the disease in places where it is not endemic, give us, if not the certitude, at all events the great probability, that it is contagious. Assuredly the opinion that cholera is non-contagious cannot be held as demonstrated. In the mean time, it is far better for the sake of science and of the public health, to regard the disease as contagious. The physician who prescribes the isolation of cholera patients, and restriction of intercourse with persons in an infected ship, for example, subjects himself to no fear of self-reproach; but he who asserts the non-contagious theory, and places cholera patients side by side with others, *may* be the cause of fatal results. M. Guyon, who has had great experience of cholera, also spoke of its nature and treatment. Our learned and *spirituel* friend (M. Velpeau), he said, gave us a most truthful definition of the disease: "It takes a man and screws him up" ("Le mal vous prend et vous tortille"). M. Guyon views the disease as essentially spasmodic—the spasm resulting from primitive lesion of the nervous system. The heart is thus, he

tells us, the first and most deeply smitten organ. "Its spasms and abnormal contractions explain all the disorders of the circulation, and the cyanosis attending the disease. In Poland, one of my colleagues gave to the cholera the name of spasm or cramp of the heart." The alvine dejections he regards in the same light as the fluids thrown off by the skin in sweating sickness. If these dejections contain, as some affirm, morbid matters, then their expulsion should be aided. Is their scantiness in very serious cases, and their absence in "dry" cholera, to be considered as a proof of their impurity or contagiousness? At all events, whatever opinion we hold, assuredly *it is not against these alvine dejections that we should direct our medication.*

Dr. Bonnafont tells the Academy of Sciences that the only efficacious remedy for cholera is a prophylactic one.

"The cholera can only be efficaciously attacked on the great Indian peninsula, or perhaps still better in London. Therapeutics have no remedy capable of neutralizing the deadly choleraic miasm. We must therefore trust to hygiene; and hygiene furnishes us with one which is undoubtedly efficacious."

The cholera seems still to linger at Malta. Some fresh cases have also occurred at Alexandria, and, in consequence, quarantine of twenty days has been placed on arrivals at Malta from that city. In Russia, cholera has appeared; and most energetic, and it is said efficacious, measures have been taken to prevent its spread. In Paris, the cholera still lingers; and there, wisely or unwisely, the number of deaths from the disease is kept from the public. *L'Epoque* says:—

"Notwithstanding our utmost desire to keep our readers correctly informed as to the progress of the epidemic, we are unable to do so, as the administration not only refuses to communicate official returns, but has warned the newspapers that any inaccuracy in the figures they might publish would render them liable to a prosecution for false news."

The number of deaths from cholera in Paris during October last is stated at 4,020; deaths from other causes being 4,274. From the 6th to the 12th instant, the deaths from cholera are put at 272.—*Brit. Med. Journ.*, Nov. 25, 1865.

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Chlorine Fumigations in Cholera.—Dr Nonat, of La Pitié, gives statistical proofs, as he considers, of the value of chlorine fumigations in preventing the spread of cholera in a hospital. The statistics are the results of observations made during the first eight months of 1854. He gives two tables, showing the number of cholera-patients brought into La Pitié and the number of cases which arose in the hospital itself, and the number of beds under different physicians.

Cholera-Patients brought into Hospital.

	Beds, Male and female.	Number of Cholera-patients.
M. Gendrin	94	173
M. Nonat	94	35
M. Valleix	80	31
M. Marrotte	96	50
M. Sée	82	40
M. Laugier	84	1
M. Michon	84	2

Patients seized with Cholera in the Hospital.

M. Gendrin	44	M. Sée	19
M. Nonat	5	M. Laugier	11
M. Valleix	17	M. Michon	5
M. Morrotte	23		

These tables show that the greatest number of cases of cholera arose within those wards into which the greatest number of cholera-patients were brought from without; but that this increase is not observed in M. Nonat's wards where fumigations were practised. Whether it may be considered as a coincidence or as a mere sequence, it is a fact (says M. Nonat) that the almost complete immunity from cholera contagion in my wards occurred from the moment of the permanent instituting of chlorine-fumigations in them.—*Ibid.*

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Isolation of Cholera Patients.—At the Parisian Hospital Medical Society, the question as to the isolation of cholera patients has been discussed.

At St. Antoine, where isolation was strictly practised during the first three weeks of the present epidemic, not a single case originated within the hospital. The cases which occurred afterwards were chiefly of attendants and in the neighbourhood of the cholera wards. Perfect isolation, said M. Bucquoy, appears to have been practised at the military hospital Gross-Caillou, and the dejections and vomited matters were disinfected. Not a single case originated within the hospital, although a great many

cases were admitted.—M. Bernutz said that, at La Pitié, thirty cases out of 157 originated in the hospital; six of these came from the surgical ward situated near the place where the beds, etc., of the cholera-patients were cleaned; one of the persons engaged in cleaning them was attacked with cholera.—M. Bucquoy mentioned striking cases to instance the transmissibility of cholera. A wet nurse returning from Paris fell ill and died of cholera at Péronne on October 28th; her husband was also attacked, and died. Five other neighbours were also attacked. Another nurse, also returning from Paris, died of cholera; her child was then given to another nurse, who also died of the disease, as well as the infant. A third nurse was also attacked. They all three belonged to the same party; and had been living several days in Paris in the Quarter of Municipal Direction of Wet Nurses, where the epidemic was then raging. The result of this was, that wet nurses from Péronne were no longer allowed to go to Paris for the purpose of bringing back with them children for suckling. These facts were all found scrupulously correct, added M. Bucquoy. No case of cholera had been previously observed in the district, and the epidemic appears to have been stifled.—M. Hérard observed that the number of cases originating in hospitals was less than in former epidemics; and he attributed this to the present isolation of cholera-patients at St. Louis, where isolation was carefully practised, very few cases originated in the hospital.—*Brit. Med Journ.*, Dec. 9, 1865.

CLINICS.

CLINICAL LECTURE.

Clinical Lecture on the Study of Children's Diseases. Given in the Hospital for Sick Children. By CHARLES WEST, M. D., Physician to the Hospital.

GENTLEMEN: A very wise and good man, to whom I owe much of whatever I have learned of my profession—Dr. Latham—makes somewhere the remark that he was struck at the outset of his career with how, in a large hospital, knowledge was continually running to waste for the want of some one to gather it. He says, too, that this which struck him then, struck him even more forcibly in after years; the old experience, in short, "*Ars longa, vita brevis*," which is realized more and more as the shadows lengthen and the day goes down.

I feel it specially with reference to this hospital, because here, or in inquiries such as its wards suggest, my time, and thoughts, and energies have been engaged for the past five-and-twenty years, and I rejoice to see you here to-day, gentlemen, because in some of you I trust that I may find fellow-labourers—men already schooled by previous study, and who will be able as well as ready to gather for the common benefit, some of that knowledge which will otherwise be but too likely lost.

But I am glad, also, to see others here, who as yet are but imperfectly trained, because, while they have much to learn, they have come here, to one of the best places in which to learn it, since disease may here be studied in its simplest forms.

It has been recommended by some, most fitted to give advice, that the student of medicine should begin with the diseases of the eye, since through its transparent coats, as through a glass, the various processes of disease and recovery may be seen transacted, and "many of the little wonderful details in the nature of morbid processes may be learned, which, but for the observation of them in the eye, would not have known them at all." The ophthalmic wards of a hospital must, indeed, be revisited at a later period for the sake of the special knowledge to be obtained there; but they may well be visited at first for the elementary teaching which they afford.

Somewhat in the same way you may come at two periods of your career to the study of children's diseases. *First*, to observe disease in its simplest conditions: then *later*, to investigate the peculiarities of symptoms which result from the tender age of your patients, and the modifications of treatment, which on that account may be required.

First, I said, to study disease in its simplest form. The chemist who analyzes a substance submits it to various processes in order to remove from it all extraneous matters, and then applies to it tests to determine its real nature. This which the chemist does, however, is very difficult, indeed, in the investigation of disease. Pure pathology is the doctrine of disease, unmodified by the intervention of disturbing causes

from without or from within. To this in adult age we scarcely ever attain. The body, even in apparent health, yet tends imperceptibly to decay. We study disease in its influence on parts already damaged. The follies of youth, the vices of maturer age, the anxieties of business, the failure of hope, all leave their impress on the body, diminish its reparative powers, and render the different organs inapt to do their duty, so that almost all disease appears in a complicated, scarcely ever in a simple form.

Care, too, which sits at the bed's head of the grown person, does much to retard recovery and to complicate disease. "Is your mind at ease?" said his physician to poor Goldsmith on his death-bed, observing how his pulse outbeat the frequency for which his bodily ailment would have accounted. In childhood there is little or none of this; no regret for the past; no dread of the future. The present is the world in which little children live; pain past is almost forgotten; and this mental tranquillity contributes in no small degree to their recovery.

But I will no longer occupy you with insisting on things with which a little time spent here will make you quite familiar. I will rather make a hasty survey of some of the cases which are now in the hospital, or which have been here so recently that many of you have had the opportunity of seeing them. I select them very much for the illustration they furnish of the unsolved problems which I want some of you to try to answer.

A little boy, aged 19 months, was admitted into the hospital, on the fourth day, of an attack of pneumonia of both lungs. His respirations were 60; his pulse beat 148 in the minute. There was dulness at the base of both lungs, especially of the right; fine crepitation was heard below both scapulæ. I scarcely need add that the child seemed very ill. He was drowsy, but at the same time restless. He was very hot, and his skin dry. He had some cough, but not very much. A mustard poultice was applied to the back and the chest. A little ammonia was given with small doses of ipecacuanha; beef-tea and wine for food.

In the night the distress and restlessness were extreme until relieved by spontaneous vomiting; but on the afternoon of the fifth day the child was already better; the respiration had fallen in frequency to 44; and there was slightly improved resonance of the chest. Improvement continued. On the ninth day the respiration had fallen to 21, and the pulse to 124; percussion yielded an almost natural sound; and some largish crepitation was the only evidence remaining of the dangerous illness.

Now here the recovery took place speedily and decisively, and in a way in which one could not refer it to the remedies employed. Nor is this a solitary case; it is one of many to which attention has of late years been especially called, which raise the question as to when and how far an expectant treatment may be adopted in inflammation of the lungs. It suggests to you the importance of determining the period of pneumonia, at which spontaneous improvement is most likely to occur, the circumstances which in any given case justify you in expecting it, and those which, on the other hand, render its occurrence doubtful. Further, there remains the important question whether, though recovery would take place independent of treatment, it yet occurs sooner, or is more complete if treatment is adopted than if the case is let alone.

A girl, 7¼ years old, was admitted with the following history. Nine months before she suffered from severe pain in her limbs, which yet did not constantly keep her in bed; but she was up every morning, and then as afternoon came on, grew worse, and went to bed. During much of this time her heart beat very much, and at the end of a month, when the pains in her limbs had already ceased, she suffered so much from her heart that she was confined to bed for six weeks. When better she attended for some months as an out-patient, but six months after her illness began, her legs swelled, her breath became short, and at length she came for admission here.

The heart's impulse was visible in the fourth, fifth, and sixth interspaces; the apex beat in the sixth interspace, one and a half inch outside the nipple line. The upper dulness limit reached to the third rib, and the inner to a finger's breadth to right of the sternum. The oblique diameter of the heart was five and three-quarter inches, the transverse five, the longitudinal three and three-quarter inches; while, as you can now see for yourselves, there is a very manifest bulging of the whole heart's region. There was a prolonged wheezing

systolic murmur heard at the apex, which diminished rapidly in loudness towards the base; the second sound was inaudible at the apex, but clearly heard at the base of the heart.

I am not going to trace this child's history in detail. She got relief from treatment, went out much better in three months, but soon came back in a state of great distress, for now pericarditis had come on. For some time she seemed likely to die, but once more got better, and you see her now, eighteen months after the rheumatism in which her sufferings began.

Now, here you have a case of heart disease, with enormous dilatation of the organ, succeeding to a comparatively slight attack of rheumatism. Each year adds to the child's sufferings, from which she will find rest only in an early grave.

Why is this so? Why does even a very small amount of valvular disease tend in some instances to produce a large amount of dilatation?

It is not an invariable occurrence. So little, indeed, is it invariable that Dr. Latham notices the probable existence of some compensating power in the young heart, by which atonement is made for the effects of valvular disease: "a certain *protective* power possibly inherent in the growing heart, whereby it can accommodate its form and manner of increase to material accidents, and so suppress or counteract their evil tendencies."

But why is this sometimes? Why not always? Why not often? Is this happy issue rarer now than formerly, and if so, can it be that the change in practice which recent years have brought with them—the abolition of depletion, the disuse of mercury, have rendered the cure of rheumatic affections of the heart less complete than formerly? Or is it only, and this I apprehend to be the case, that our diagnostic skill and pathological knowledge have outstripped by far our therapeutical resources, that we discover the ills which we are impotent to cure?

A strong-looking, well-made girl, 10 years and nine months old, began to suffer causelessly from chorea three weeks before admission into the hospital. There was no history of rheumatism in her family, nor had she herself presented any rheumatic symptoms, though there was a weak systolic bruit audible at the apex of her heart, which persisted, but did not increase either in extent or in loudness during the whole of her illness. The choreic movements were at first limited to the left arm, but they increased rapidly in spite of treatment, so that a month after admission the child was compelled to be placed in a bed padded all round, on account of the violence of her movements, while deglutition was very imperfect, and speech almost abolished.

She remained in the hospital for three months; at the end of which time she was almost well, and was sent into the country. She was submitted to very varied treatment, but without benefit, and her eventual improvement was spontaneous. For a time she improved in the country, but at the end of two months returned with a relapse of all her former symptoms, though their severity was far less than on the former occasion. In this instance medicine seemed just as unavailing as before. The child began spontaneously to improve at the end of one month, was well at the end of two months, and has, I believe, since continued so, though the time is yet too short to feel sure of the permanence of her recovery.

Here, again, are several questions which await an answer. Why is the first attack of chorea almost always the most severe? Why is there no definite relation between the severity of chorea and the severity of the heart affection, and why is the heart sometimes quite unaffected, even though the chorea is very severe? Lastly, why is the heart affected at all, since the assumption of its rheumatic character, though true to a certain extent, is yet by no means always tenable?

Further, what clear indications can be laid down for the treatment of chorea besides the two furnished by the existence of constipation on the one hand, and anæmia or debility on the other, and the combined use of purgatives and tonics which they suggest?

Zinc and antimony, strychnine and belladonna, shampooing and sulphur baths have all been used in the treatment of chorea. When is the one right, when the other, or in what combinations are they best employed? In what combinations? for I would not have you fall into the error into which the prevalent folly of homœopathy may imperceptibly lead you, of supposing that in order to act at all each remedy must be employed alone. He is the best physician, who knows best not only what remedies to

use but in what combinations; as the skilful general trusts not to his infantry alone, nor alone to his cavalry, but gains his victory most surely and most quickly by using different troops in combination; or,

"As many arrows loosed several ways
Fly to one mark."

Two more cases, and I have done for to-day. One is in the hospital now, the other recently left.

A boy, 8½ years old, was always a backward child, and while teething had three attacks of convulsions. Three months since he seemed causelessly languid for a fortnight: and then he was suddenly seized with vomiting. For six weeks together the vomiting returned daily or every other day. It was associated with an increased languor, and, by degrees, with drowsiness, and with pain in the occiput, which, though constant, became sometimes so severe as to make him scream aloud. A month after the commencement of these symptoms he was first observed to squint, and at the end of two months he had a fit which lasted for half an hour. In the ensuing month these fits returned six times. The vomiting ceased after the first fit, but the other symptoms continued, and became associated with pain on any movement of the limbs, and three weeks before his admission into the hospital it was first observed that his pupils were dilated, and that he had lost the power of sight.

He was a pale, thin child, with a peculiarly wretched expression of countenance; absolutely blind—his right eye looked straight forwards, his left inwards, and both were in a state of constant motion. He had complete power over his limbs. His headache was not constant; his appetite was good, and he did not vomit during the fortnight that he remained in the hospital. Nothing, however, seemed for a moment to amuse or please him, and he was allowed to go home all the more readily that his case was not one which held out much prospect of benefit from treatment.

What was this case? There was no family history of tubercle, nor did the boy present any appearance of it. Still the symptoms are not those of any acute inflammatory disease, and I should be disposed to imagine that they were due to the gradual development of some tumour (and these tumours are almost always tuberculous), which, arising at the base of the brain, had, by degrees, increased until by pressure on the optic nerve it had abolished the power of vision. And here it may remain stationary, though more probably it will continue to grow until it causes death, either suddenly by some outpouring of blood from the vessels at the base of the brain, or more slowly, by the production of inflammation, or by effusion, into the ventricles consequent on pressure on the veins of Galen.

Here is, lastly, another case, somewhat obscure, indeed, but yet less so, I take it, than the preceding one:—

A girl, 8½ years old, whose father and two of her brothers had died with symptoms of brain disease, had suffered for a fortnight from troublesome cough, when she seemed unusually heavy, was attacked by violent sickness with headache, and sank speedily into a state of stupor, which continued with intervals, during which her mind wandered, and she rambled in her talk for thirty-six hours. At the end of this time consciousness returned, and the child sat up in bed, and showed some gleams of cheerfulness, but the pulse, which had been irregular during the state of stupor, still continued so, and the head was held somewhat retracted. Pain in the head and some retraction of it continued, though the child was well enough to be up, and moved about the ward.

She left by her mother's wish in a fortnight, and at the end of another fortnight she returned, much emaciated, complaining of pain which was now referred more to the ears than to the head, of pain also in the neck and in the right shoulder, towards which her head was inclined.

On this occasion she remained in the hospital four weeks. During this time she grew thinner and thinner, her skin became harsh, her abdomen retracted and tender to the touch, and her head was still drawn back as before. Auscultation now found the breathing weak everywhere, but especially so at the apex of the left lung, and percussion there was obviously dull. There was no vomiting, however. The bowels, once constipated, had now become regular, the complaints of headache were less constant, and the pulse had lost its irregularity. General tuberculosis was advancing; the mischief in the brain, I suppose, was stationary.

What is the import of this sudden development of the signs of cerebral disease, and what of their spontaneous passing into

abeyance? If we could learn to answer these inquiries aright we might possibly do something to arrest disease, even though we were unable to effect its cure. Here, then, is another problem which I leave for your consideration.

But, say you, you came here to be told what I do know, and I have talked to you almost entirely of what I do not know, and that is not the object of a lecture, the purpose of which should be to impart positive knowledge.

Gentlemen, it is not quite so. The acquisition of knowledge implies an active, not a passive state, and to this it was my object to excite you. It is when you seek as for riches, and search as for hid treasure, that you gain it; so, at least, said the man wiser than other men, and who himself wrote of all things, from "the cedar of Lebanon to the hyssop that groweth on the wall."

You have come to the study of medicine, furnished far differently from those who, like myself, entered on it more than thirty years ago. It is but right that you should turn these advantages to good use. We are, indeed, as has been well said, like people standing together on a hill, which I have climbed before you, and I, to whom the landscape is in some measure familiar, may say to you, look here and look there, and you will see this and that. But further, I say to you, the objects there are indistinct to me; but you have perspective glasses of higher power than mine; turn them in this direction or in that, and you may with patience discern clearly what I can see but partially, or, with my imperfect instrument, perhaps cannot see at all.

If you visit the wards of this hospital, I may, too, do some of you the service, that I may point out to you what is worth the seeing, and may help to guard you from the dangers of the young student—that of playing with the instrument itself, vain, perhaps, of his dexterity in its use, or of turning it thoughtlessly on trifling things not worth the investigation.

You must not forget that it is your duty, as it is mine, to map out the country for the use of yourselves and of future travellers, to seize its great features, which may serve as landmarks, and not to waste your time on some quaint tree or curious rock which lies quite out of the path along which you have to journey.

"Nisi utile est quod agimus vana est gloria nostra" should be your motto, though in a different and a lower sense, indeed, from that in which it was employed by the inspired penman some eighteen hundred years ago.—*Med. Times and Gaz.*, Oct. 28, 1865.

HOSPITAL NOTES AND GLEANINGS.

Case of Ligature of the External Iliac Artery.—On November 17 Mr. ADAMS tied the external iliac for the cure of an aneurism of the common femoral artery. The case was that of a woman, aged 53, and was peculiar from its being supposed to have originated from a kick in the groin received three years ago. A swelling resulted from the violence inflicted, and remained stationary until five months ago, when it began to enlarge until it increased to the size of two fists, and extended into the pelvis at least three inches. The limb was much swollen and very tense in some parts, owing to the pressure upon the vein. The minute capillaries of the skin were much distended.

An incision of at least five inches was made in the usual situation, and the layers of abdominal muscles and the fascia transversalis were carefully divided, and the artery was readily secured.

The case has progressed satisfactorily to the present time, the tumour has much diminished, and the leg has returned to its natural shape and size.

The ligature came away on the twenty-ninth day.—*Med. Times and Gaz.*, Dec. 23, 1865.

MEDICAL NEWS.

DOMESTIC INTELLIGENCE.

Council of Hygiene, Citizens' Association of New York.—This council, the officers of which are: *President*, Joseph M. Smith, M. D., *Vice-President*, Willard Parker, M. D., *Secretary*, Stephen Smith, M. D., with twelve of the most eminent physicians of the city as members, have proposed the following organization for effecting sanitary improvements of towns, and for the prevention and mitigation of cholera.

"*Whereas*, The commercial and social relations of the cities, villages, and communities of the State have become so intimate

by the great facilities for travel, that contagious, infectious, and epidemic diseases are rapidly and widely disseminated from populous towns, where they are fostered and frequently generated by the neglect of sanitary works, to other towns along the principal public thoroughfares, and to the most remote rural districts; and

"*Whereas*, Cholera, the most fatal of modern epidemics, and soon apparently to visit this country, is governed by the same law of progression as other pestilential diseases, spreading from town to town along the great routes of travel, and selecting the filthy and unclean districts of cities for its ravages; and

"*Whereas*, Experience teaches that the communicable diseases of towns and epidemic cholera may be controlled, and frequently suppressed, by the timely and rigid enforcement of proper sanitary regulations; and that when the latter is prevailing its severity may be greatly mitigated, and its fatal issues prevented by the prompt and efficient treatment of its premonitory symptoms; therefore

"*Resolved*, That the people of this State have a common interest in the public health of its cities and villages; and, for their own safety, as well as for the public good, should, with united effort, sustain every measure designed to effect sanitary improvements of towns, and to prevent the spread of pestilential diseases therefrom.

"*Resolved*, That in view of the approach of Asiatic cholera, we urge the medical profession of the State of New York to form voluntary organizations in every city, town, village, and community exposed to an attack of the epidemic, for the purpose of effecting, *first*, needed local sanitary improvements; and, *second*, to organize a corps of house-to-house visitors, who, in the event of the prevalence of cholera, shall visit the poor at their homes daily, and search out and promptly treat all cases of premonitory diarrhœa.

"*Resolved*, That all such organizations be invited to correspond directly with this council, and with each other, in order to concert of action in the adoption and prosecution of measures of prevention and mitigation of cholera, and for the purpose of a systematic study of its various phenomena over a large field."

The council, in the prosecution of their objects, "has organized a corps of Medical Sanitary Inspectors, thirty-one in number, who have made a thorough inspection of the city, the results of which have been published in the form of a report to the Association.

"With this corps of inspectors, properly augmented, the Council propose, in the event of the appearance of cholera, to undertake a systematic house-to-house visitation, with a view to the early treatment of the premonitory diarrhœa at the homes of those affected. This plan is extensively adopted in England, and there is evidence that in former visitations of cholera many towns were saved from a single fatal case, though thousands of cases of diarrhœa were treated.

"It is the hope of the Council that similar voluntary associations will be made in every city, town, village, and community, in order that the Medical Profession of the State may present a united front against the invasion of the epidemic; and, in the event of its breaking out in any locality, may have in readiness for immediate and effective action, an organization that will control its progress and mitigate its severity."

The prompt and judicious action of the profession in New York city in this matter cannot be too highly commended, and we most earnestly urge upon the profession here and elsewhere throughout our country the adoption of a similar course.

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American Sanitary Museum of Dr. Thomas W. Evans.— [Dr. Thomas W. Evans, now of Paris, desiring to benefit mankind generally, and to confer honour on his native country by making known abroad a great number of inventions, made by his countrymen for relieving sick and wounded soldiers, has issued the following circular, to which we invite attention:—]

"Penetrated with the idea that the Sanitary Commission of the United States, by mitigating the horrors of war, had resolved one of the most urgent questions of modern time, I was one of the first persons in Europe who endeavoured to acquaint the public with the organization and the results of that admirable institution. I first published a book ('La commission sanitaire son origine, son organization et ses résultats') in which I conscientiously exposed the efforts and the final success of the Sanitary Commission during the gigantic struggle that the United States sustained with un-

abated courage. Afterwards appeared my French translation of military, medical and surgical essays. By acting so I felt I was serving both the cause of humanity and that of my native country.

"After having shown the wonderful results of the Sanitary Commission, it would be just and proper now to acquaint the public with the great number of ingenious inventions, made by my countrymen in view of relieving the sick and the wounded soldiers

"In order to realize this project; I intend to assemble in a collection the products of those inventions which have enabled the Sanitary Commission to fulfil its mission.

"The universal Exhibition that is to be opened in Paris in 1867, is certainly the best opportunity for the inauguration of this Sanitary Museum. During that exhibition no civilized nation will be unrepresented in the French metropolis. The articles exhibited in such a Museum will therefore call the attention of all those who wish the welfare of mankind, and acquaint all nations with the name of their inventors.

"In addressing myself to my countrymen I am firmly convinced that they will assist me in my patriotic and humanitary enterprise. Although I am willing to purchase all such articles as may be useful, I shall gratefully accept any object that the inventors or manufacturers would wish to contribute.

"I therefore most respectfully request all such persons who are disposed to co-operate in the creation of the American Sanitary Museum, to address their communications to Dr. Thomas W. Evans, 15 rue de la Paix, Paris (France), or to M. Abner L. Ely, 22 Pine Street, New York.

THOMAS W. EVANS, M. D.,
15 *rue de la Paix.*
Paris, Dec. 1, 1865."

FOREIGN INTELLIGENCE.

Death from Chloroform.—At a recent meeting of the Berlin Medical Society, Dr. Hüter related a case of death from chloroform which occurred in the Surgical Polyklinik. A boy 4½ years of age, who some time since had an attack of scarlatina followed by œdema of the extremities and albuminuria, was brought on account of retention of urine, none having been passed, according to his mother's account, for two days. Slight œdema still existed, and the child looked pale and ill. On examination there was found to be distension of the bladder, but the boy was so restless while it was made that it was deemed prudent to administer chloroform before passing the catheter. This was given by an assistant accustomed to its exhibition, a napkin being moistened with a small quantity—this being, in fact, less than children of this age usually require, and not to be compared to that employed in operations. The abdomen was examined again, and a catheter was passed into the bladder, from two to three minutes at most having elapsed since it was determined to employ chloroform. At this moment, however, the lips were observed to become blue and the jugulars distended, while the pulse and respiratory movements ceased. A deep inspiration was caused by passing the finger down to the epiglottis, but this could not be renewed; a second one, however, occurring upon opening a vein of the neck and discharging an ounce or two of blood to relieve the turgor. Tracheotomy was now resorted to (a third inspiration occurring during its performance) and artificial respiration performed, the diaphragm being at the same time stimulated by the induction apparatus, and the face kept sprinkled with cold water. Electricity was also applied in the region of the heart, and this was followed by acupuncture of the organ by means of two long needles. The heart's action, which had ceased to be audible, was seen by the regular and isochronous movements of the needles to become temporarily revived. These, however, soon ceased. All these means were most energetically applied by able assistants, and Dr. Hüter does not see that in a similar case that he could do otherwise, excepting, perhaps, that he would resort to acupuncture of the heart at an earlier period. No account of the autopsy is furnished. He does not think that so exceptional a case of death from chloroform should influence our general employment of this agent—this, indeed, being the first instance on record of a child under five years of age dying from the effects of chloroform. Deaths under chloroform may be ranged in three categories—those in which this agent really exerts no essential effect, the patient dying not through, but during, the narcosis, in consequence of some other circumstance accompanying or preceding the operation; those cases in which the

narcosis only operates mediately—*e. g.*, through the flow of blood into the anæsthetised glottis during operations on the throat; and those cases in which the narcosis is the direct cause of death. It was so in the present case, although the patient had inhaled so little of the chloroform the entire purity of which has been ascertained.—*Med. Times & Gaz.*, Jan. 6, 1866.

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Epidemic of Trichinosis.—An eye-witness gives, in a recent number of the *Deutsche Klinik*, an interesting account of a severe epidemic of *trichinosis* which occurred at Hedersleben, in Prussian Saxony—a place containing about 2,000 inhabitants. A butcher having slaughtered two pigs, most of the consumers, among whom were many factory hands, ate the chopped flesh in a raw condition; and soon afterwards many persons at the factory were seized with a similar illness. One of the doctors of the town immediately suspected trichinæ, but others called in consultation declared the affection to be cholerine. However, as out of 37 cases 20 soon died one after the other, the autopsies amply proved that the disease really arose from trichinæ; and three weeks after the first case the whole number had amounted to 300, with 40 deaths. (According to another account, there were 100 deaths.) This does not represent the whole numbers, for several persons, alarmed by the cholera panic, left the town, and of these several have died at the places they repaired to. At the autopsies, even three weeks after the outbreak, numerous "parent trichinæ" were found in the intestinal mucous membrane, the great bulk of the animals being in the intramuscular structure. The number of children affected, going down even to the third year, was remarkably great, yet they all recovered. As on other occasions, some of the patients, though exhibiting all the characteristic symptoms, denied having eaten pig's flesh, while some admitted only to have partaken of the fat. The immediate cause of death seemed to be paralysis of the inspiratory muscles; the most alarming symptoms, besides the loss of motion, were the profuse sweating and complete sleeplessness. There was delirium only in one case, and the convalescence resembles that of typhus. It seems that in examinations which have been recently made of beetroot, which forms the principal food of pigs, numerous animalcules have been found, but these are said to present quite different characters to those of the trichinæ.—*Med. Times & Gaz.*, Jan. 6, 1866.

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Etherization.—M PETREQUIN, of Lyons, brought the subject of *etherization* the other day before the Academy of Sciences. It seems that he and almost all the Lyonese surgeons have adopted ether in preference to chloroform during the last fifteen years, and have met with no fatal cases or serious accidents, while complete anæsthesia has been promptly and effectually secured. Its free adoption at first was impeded by three circumstances, which no longer prevail. 1. The defective and complicated character of the apparatus employed. These have been now superseded by a simple contrivance, termed a *sac a éthériser*, which is admirably efficient. 2. The ether employed at first was of insufficient strength and impure. A strong, pure, concentrated, rectified ether at 62° or 63° is now sold for the purpose at the Lyons' *pharmacies*. 3. The inexpertness of the early manipulators is now exchanged for a dexterity which induces quiet and speedy etherization; while a careful observation of the pulse and respiration secures the patient from all accidents, which are easily averted, when threatening, by temporary suspension of the inhalation. Such accidents, under ether, are always progressive, and not sudden, as under chloroform. They may be anticipated or arrested, and never present themselves with the formidable rapidity characterising some of those induced by chloroform. M. Velpeau, while believing that the statement of the entire innocuity and prompt utility of ether made by so important a body as the Lyons practitioners is highly deserving of the attention of the Academy, does not think the argument for its preference over chloroform at all conclusive. Many of the dangers attaching to this latter agent may also be due to its impurity or unskilful application; but, speaking from his own experience, he has employed chloroform in many thousand cases during the last fifteen years, without ever meeting with a fatal case. This, too, is the case with many of the surgeons in the best practice in Paris, and with the entire school of Strasburgh. In fact, either agent may have its useful application under different circumstances.—*Med. Times and Gaz*, Dec. 23, 1865.

Increased Use of Stimulants in the London Hospitals.—Some remarkable statistics regarding the employment of stimulants and the mortality in the London Hospital during some past years, appear in the last volume of the *Reports* of that hospital. In 1862, the number of in-patients was 4,519, and the general mortality 7.6 per cent. The quantity of stimulants consumed was 1,281 gallons of wine, 162 gallons of brandy, 38 gallons of gin, and 1,100 ounces of cinchonine.

In 1864, the number of patients was 4,619, and the general mortality 10.5 per cent.; the stimulants consumed by these being 1,558 gallons of wine; 359 gallons of brandy; and 77 gallons of gin. But as a set-off, if it may so be called, 760 more leeches were employed during this year than the average for the five preceding years; viz, 3,840. However, here we have a great increase in the amount of stimulants consumed, and also a great increase in the mortality of 1864 as compared with that of 1862. We state the facts, let it be understood, without in any way pretending to connect them as cause and effect.

Other statistics Dr. Fraser gives us under this head. "From 1854 to 1858, the annual average quantity of wine employed by each physician was 12,803 ounces;" each physician having an annual average of 391 patients under treatment. The annual average mortality was 11.87 per cent. But from 1860 to 1864, the annual average quantity of wine employed by each physician was nearly quadrupled, being 48,136 ounces; his annual average number of patients was 413; and the annual average mortality was 12 65 per cent.

From 1854 to 1858, each surgeon employed annually 38,016 ounces of wine; his annual number of patients was 1,036, and the average annual mortality 4.48 per cent.

From 1860 to 1864 (five years), each surgeon employed an annual average of 142,951 ounces of wine (nearly four times more than in the previous years); the annual number of patients under him was 1,065, and the annual average mortality 6.65 per cent.

Hence we have, in the practice of both physicians and surgeons, a distinct increase of mortality coincident with great increase in consumption of stimulants.

Dr. Fraser also tells us (referring to a former paper of his) that, in 1851, there were 4,051 in-patients in the London Hospital; that in 1857, there were 3,935 in-patients; and that the mortality was greater in 1857 as 8 to 6.5 per cent., although £962 more were spent in 1857 than in 1851 for articles of luxury.

It is curious to note, that the only comment which Dr. Fraser makes on the above remarkable statistics is this:—

"It is evident, that a steady rise in the employment of stimulants..... is still going on; and whatever be the cause, we may rest assured that the practice is imperative and needful; for it would be a monstrous assumption that a whole staff could be blindly following an objectless routine."

Not a single word of comment does Dr. Fraser bestow on the constant fact of the coincident increase of the mortality!

The summary of these statistics stands thus:—

From 1854 to 1858, each physician employed 12,800 ounces of wine annually; the deaths being 11.88 per cent. From 1860 to 1865, he employed 48,136 ounces; the deaths being 12 65 per cent.

During 1854 to 1858, each surgeon employed annually 38,016 ounces of wine; the deaths being 4.48 per cent. During 1860 to 1864, he employed annually 142,951 ounces; the deaths being 6.65 per cent.

In 1862, the general mortality of the hospital was 7.4 per cent.; the consumption of stimulants being 1,281 gallons of wine, 162 of brandy, and 38 of gin.

In 1864, the mortality was 10 5 per cent.; the quantity of stimulants consumed being 6,558 gallons of wine, 359 of brandy, and 12 of gin.

We again repeat, well knowing the sad fallacies which are so often edited through an erroneous interpretation of statistics, that we do not pretend to connect the increase of deaths with the increase of stimulants consumed. But, when we reflect upon our modern advancement in medicine and surgery (especially as miscalled conservative), when we think of our great modern hygienic efforts, we may fairly ask for some explanation of the fact of a general advance in the mortality of a London Hospital.—*Brit. Med. Journ.*, Dec. 9, 1865.

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Baron Liebig on Coffee.—Baron Liebig, in an a recent No. of the *Popular Science*

Review, gives the following directions for making this beverage:—

First of all, coffee should always be sorted. It frequently contains foreign substances, and usually a number of black mouldy berries; these spoil the flavour of the coffee. Berries of dark or green hue are generally dyed. They should be washed in a little warm water, and afterwards dried with a warm linen cloth. In order to retain the volatile caffeine, the berries should only be roasted till they are of a pale-brown colour. In those which are too dark there is no caffeine; if they are black, the essential parts of the berries are entirely destroyed and the beverage prepared from them does not deserve the name of coffee. When roasted, they constantly deteriorate by the access of air. If, when the roasting is completed, half an ounce of white or brown sugar be strewn over each pound of coffee, it gives the berries a fine glaze, which protects them from the atmospheric destruction. The usual methods of preparing coffee are classified by Liebig as—1st, by *filtration*; 2d, by *infusion*; 3d, by *boiling*. Filtration partially injures the aromatic particles, and makes an incomplete extraction. Infusion gives a very aromatic coffee, but one containing little extract. Boiling gives a coffee rich in extract, but poor in aroma. Here, then, is Liebig's compound method:

"The usual quantities both of coffee and water are to be retained; a tin measure containing half an ounce of green berries, when filled with roasted ones, is generally sufficient for two small cups of coffee of moderate strength, or one, so called, large breakfast cup (one pound of green berries, equal to sixteen ounces, yielding after roasting twenty four tin measures [of half an ounce] for forty-eight small cups of coffee).

"With three-fourths of the coffee to be employed, after being ground, the water is made to boil for ten or fifteen minutes. The one quarter of the coffee which has been kept back is then flung in, and the vessel immediately withdrawn from the fire, covered over, and allowed to stand for five or six minutes. In order that the powder on the surface may fall to the bottom, it is stirred round; the deposit takes place, and the coffee poured off is ready for use. In order to separate the dregs more completely, the coffee may be passed through a clean cloth; but generally this is not necessary, and often prejudicial to the pure flavour of the beverage.

"The first boiling gives the strength, the second addition the flavour. The water does not dissolve of the aromatic substances more than the fourth part contained in the roasted coffee."

This coffee, says Liebig, must be judged not only by its agreeable qualities as a beverage, but by its excellent effects on the organism. There are many other interesting details in the paper, for which we must refer our readers to the source abovenamed.—*Lancet*, Jan. 6, 1866.

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Oxidation of Fatty Vegetable Oils.—M. Clöez has exposed oils to the air in colourless glass vessels, and also in vessels of red, yellow, green, and blue glass, and also left some oil exposed to air in total darkness. After ten days' exposure the increase of weight was greatest in the colourless glass vessel; it was rather less in the blue glass; was very small in the red, yellow, and green; and no increase of weight at all was observed in the oil exposed in the dark. Like results were found after twenty days; but after thirty days' exposure the results were somewhat different. The increase of weight was greater in the coloured glasses than in the uncoloured, green showing the largest increase after 150 days' exposure. Poppy oil, after a time, oxidized faster in the dark than when exposed to coloured or white light. Oil heated in atmospheric air oxidized much more rapidly than cold oil. The oxidation may be accelerated without heat by adding some oil already oxidized.—*Ibid.*, from *Chem. News.*

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M. Jobert de Lamballe.—We learn with regret from our recent journals that this eminent surgeon is suffering from mental derangement which has necessitated his removal to a lunatic asylum.

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Obituary Record.—Several medical men have succumbed at Madrid from cholera. Professor Quintanilla, and Drs. Avilès and Montana, two of the leading physicians of the city, have fallen victims to it; so also have MM. del Pozo and Andrès, attached to the Ambulance; and Gabridra, the Dean of the Pharmaceutical Faculty pon y Camps, and the *Pharmacien* Iniguez, have been carried off. At Cadiz, Dr. Gabarron, Professor of Surgery, died of cholera.